THE HUXLEY LECTURE.—RECENT ADVANCES IN SCIENCE AND THEIR BEARING ON MEDICINE AND SURGERY.[1]

I.

WHEN fifty-four years ago the school of Charing Cross Hospital gathered itself together for its winter work, among the new comers was a pale-faced, dark-haired, bright-eyed lad, whose ways and works soon told his fellows that he was of no common mould. To-day I am about to attempt the fulfilment of the duty, which the authorities of the school have done me the honour to lay upon me, of delivering the first of the series of lectures which the school has wisely instituted to keep alive, in the minds of those to come, the great services which that lad's strenuous and brilliant life rendered to the healing art. The trust of the Huxley Lectureship provides that the lecturer shall dwell on recent advances in science, and their bearings on medicine and surgery. I venture to hope that I shall be considered as not really departing from the purpose of the trust, if I attempt to make this first lecture a sort of preface to the volume, or rather the volumes of lectures to come. And since a preface bears a different paging, and is written in a different fashion from that which it prefaces, I shall be so bold as, with your permission, to make the character of my lecture to-day different from what I suppose will be that of the lectures of my successors. It will, I imagine, be their duty to single out on each occasion some new important advance in science, and show in detail its bearings on the art of medicine. Each succeeding lecturer will, in turn, be limited in the choice of his subject, and so assisted in his task by the choice of his predecessors. I to-day have no such aid. It seems fitting that, for the purposes of this initial lecture, the word "recent" should be so used as to go back as far as the days of Huxley's studentship. If it be so used, I am brought to face advances in science affecting medicine and surgery, so numerous and so momentous that any adequate treatment of them as a whole would far exceed not only the time at my disposal, but also, what is more, my powers to treat and your patience to hear. I will not dare so hopeless a task. Nor will I attempt to select what may be deemed, or what may appear to me, the most important of these advances, and expound the bearings on medicine of these alone. I venture to hope I shall best fulfil the duty laid upon me, and meet with your approval, if I single out and dwell on one or two general themes suggested by the history of science during those fifty odd years.

The first theme is one suggested by a survey of the studies which engaged young Huxley in the school here in 1842. This will bring before us a special bearing, on our profession, of the advance of science, which, though it may not be evident at first sight to every one, is nevertheless real and important.

Each case of illness is to the doctor in charge a scientific problem to be solved by scientific methods; this is seen more and more clearly, and acknowledged more and more distinctly year by year. Now it is true that each science has to a certain extent its own methods, to be learnt only in that science itself; and from time to time we may see how a man eminent in one branch of science goes astray when he puts forward solutions of problems in another branch, to the special methods of which he is a stranger. In nothing is this more true than in an applied science like that of medicine. At the bedside only can the methods of clinical inquiry be really learnt; it is only here that a student can gain that kind of mind which leads him straight to the heart of disease, that *genius artis*, without which scientific knowledge, however varied, however accurate, becomes nothing more than a useless burden or a dangerous snare. Yet it is no less true that the mind which has been already sharpened by the methods of one science takes a keener edge, and that more quickly, when it is put on the whetstone of another science, than does a mind which knows nothing of no science. And more than once inquiry in one science has been quickened by the inroad of a mind coming fresh from the methods of a quite different science. For all sciences are cognate, their methods though different are allied, and certain attitudes of the mind are common to them all. In respect to nothing is this more true than in respect to the methods of medicine. Our profession has been the mother of most of the sciences, and her children are ever coming back to help her. In our art all the sciences seem to converge—physical, chemical, biological methods join hands to form the complete clinical method. This is the real justification for that period of preparatory scientific study, which each enactment of the authorities makes longer and harder for the student of medicine. It is this, and not the mere acquirement of facts. The facts, it is true, are needed; every day the doctor has to lay hold, for professional use, of mechanical, physical, chemical, biological facts. But facts are things which the well-trained mind can pick up and make use of as it goes along at any time and in any place. Whereas the mind which is not well-trained will miss the facts or pick up the wrong ones, or put to a wrong use even the right ones which it has in hand.

Now the ideal training to be got from any science is that of pursuing inquiry within the range of the science, according to the methods of the science; in that way only does the spirit of the science fully enter into the man. But such an ideal education is impossible. We are fain to be content in merely making the student know what truths in each science have been gained and how they have been gathered in, such a teaching becoming more and more effective as a training, the more fully the student is made to tread in the very steps, and thus to practise the methods of those who gained the truths.

The more complete the body of any one science the more useful does that science become as a means of training, and hence it is that advance of science has a double bearing on the medical profession. As each science grows, not only does its new knowledge bring to the doctor new facts and new ideas, new keys to open locked problems, and new tools to use day by day, but the incorporated knowledge gains greater and greater power as an instrument to train his mind rightly to use all the facts which come before him.

Let me, in the light of this view, call your attention for a moment to the yoke of compulsory studies under which the young Huxley had to bend his somewhat unruly neck, and compare it with the like yoke which presses, heavily it seems to some, on the neck of the young student of to-day.

I have not been able to find an exact record of the course of studies pursued by Huxley himself at Charing Cross in the years 1842–5, but I have been privileged to examine the stained and tattered schedule of the College of Surgeons, duly "signed up," for the years 1844–7, belonging to one who, during some of those years, sat by Huxley's side, who was then, and afterwards, his friend, and who has won honour for himself and for your school, under the name of Joseph Fayrer.

I find that young Fayrer attended during his first year a course of at least 140 lectures with 100 demonstrations on Anatomy and Physiology, a course of not less than 70 lectures on Materia Medica, a course of lectures on the Practice of Surgery, and a course of "The Practice of Physics," each of not less than 70 lectures, and a course of Hospital Practice in Surgery of not less than nine months. In his second year he again attended the 140-lecture course on Anatomy and Physiology, and the 70-lecture course on the Practice of Surgery, and again Hospital Practice in Surgery, taking as well a 70-lecture course in Chemistry, a like course in Midwifery and Hospital Practice in Medicine. In his third year he once more attended the 140-lecture course in Anatomy and Physiology, but no other systematic lectures; the rest of his time was devoted to Hospital Practice. To these demands of the College of Surgeons we ought to add, in the case of the ordinary student, the demands of the Company of Apothecaries; but the main addition thus caused would be a course of Botany.

Such a curriculum differs widely both in nature, extent, and order from that in force at the present day. But I venture to think that if we examine the conditions of the time, we shall find that the authorities of that day were as wise as, possibly wiser than, we of to-day. In judging such matters as these, we and, perhaps, especially they who would drive the student on into learning by the goad of compulsion, must bear in mind that legislative enactments, such as those prescribing a curriculum of study, always exhibit a long latent period; they come into visible existence long after the stimulus which begat them has been applied, long after the need of those things being done which the enactments strive to do has been felt. So long, indeed, is the latent period, that often new needs have arisen calling for yet other regulations before the old ones appointed to meet the old needs have got into working order. Bearing this in mind, we shall find that the course of study prescribed in Huxley's time was wisely chosen to meet the needs of, at least, the time immediately preceding that, if not, indeed, the time itself.

It will be observed that the study of physics, or as it was then more commonly called natural philosophy, finds no place whatever in young Fayrer's schedule, and that the one short course of chemistry, without any practical instruction, which he attended was taken in his second year—in the middle, as it were, of his curriculum, when he was already advanced in his clinical studies.

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[1] Delivered at Charing Cross Medical School, on October 5, by Prof. Michael Foster, Sec.R.S.

At the present time the sciences of physics and chemistry have each of them developed into a body of logically coordinate truths, furnishing an instrument of peculiar value for the training of the scientific mind. Moreover the methods of teaching have developed in no less a degree, so that in the laboratory the student follows, at a long distance, it is true, but still follows the steps of those who have made the science, and has at least the opportunity of catching something of the spirit of scientific inquiry. In this educational value of these sciences, even more than in the practical utility of a knowledge of the mere facts of the sciences, great as that may be, lies the justification of the authorities when these, desiring to improve the profession by introducing artificial selection into the struggle for existence, insist that all to whom the lives and health of their fellow men are to be entrusted, should have learnt at least something of the sciences in question.

In the time of Huxley's studentship both these sciences were in a very different condition. The time, it is true, was one of great awakening. In physics men's minds were busy opening up the hidden powers of electricity: some ten years before Faraday had made an epoch by discovering induced currents; he and others were still rapidly extending our knowledge, one practical outcome of which was the introduction of the telegraph in 1837. But how great has been the onward sweep in electric science since then; how great the advance in all branches of physics! To realise the great gap which separates the physics of to-day from the physics of then, one has only to call to mind that the world had yet to wait some years before Mayer, and Joule, and Helmholtz, and Grove had said their say; in the books which taught young Huxley the laws of physics he found not a word of that great law of the conservation of energy, which like a lamp now guides the feet of every physical inquirer, whatever be the special path along which he treads.

In chemistry much, too, was being done. That science was in the first flush of success in its attack on the mysteries of organic compounds. Liebig, Dumas, and others were rapidly making discoveries of new organic bodies, and dealing with types and substitution, were beginning to make their way into the secrets of chemical constitution; but then, as indeed for a long time afterwards, progress was taking the form of the accumulation of new facts interesting and eminently useful, but still mere facts, rather than of the gaining of insight into those laws of chemical change of which the facts are but the expression. And the brilliant success of purely organic chemistry was somewhat prejudicing those inquiries in regions where physics and chemistry touch hands, which in these latter days are producing such striking results.

In the days of Huxley's studentship neither of these sciences presented such a body of truths as could be readily used as an engine of mental training, nor had the educational mechanism for thus employing them been developed; a chemical laboratory for the student was as yet hardly known, a physical one wholly unknown. The profession turned to these sciences chiefly for the utility of the facts contained in them. The facts of physics, with the exception of those of mechanism, were but rarely appealed to, and if those of chemistry were in more common use, it was because they threw light on the mysteries of the Pharmacopœia, rather than because they helped to solve the problems of the living body. Hence the authority, not without cause, demanded of the student no physics at all, and asked for chemistry only in the midst of his course, when its facts might help him to understand the nature of the drugs which his clinical studies were already bidding him use.

As regards the biological sciences, the time was also one of change, or rather of impending change; the causes of the change were at work, but for the most part were at work below the surface; their effects had not yet become obvious.

In natural history, in what we sometimes now call biology, in botany, zoology, and comparative anatomy, the activity in systematic and descriptive work was great. The sun of the great Cuvier was setting, but that of our own Richard Owen was at its zenith; new animal forms, recent and extinct, were daily being described, the deep was giving up its treasures, new plants and new beasts, brought home by energetic travellers, were being duly investigated. But this was only a continuation of what had been going on long before.

Of the great biologic revolution which was about to come, there was not so much as even a sign in the skies when Huxley took his seat on the Charing Cross benches, though Charles Darwin was already brooding over the ideas which had come to him in his long voyage.

Two great changes, however, were already beginning—one due to new ideas, the other to improved methods.

The morphological conceptions, of which von Baer, in his "History of Development," had laid the foundations, destined to make a new science of animal forms, were being carried forward by Johannes Müller in Germany, though, save for the expositions of Carpenter, they had made but little way in this country. Nowhere, indeed, had they progressed far. The man who, perhaps to Huxley himself, was to advance them most, Gegenbaur, was as yet a mere student. Nor in spite of the beginning made by von Baer himself, by Allen Thomson, and by Rathke, had embryology made much progress. Kölliker, to whom the science owes so much, had as yet written no line. Still the new ideas were beginning to push.

Of no less importance was the impulse given by the improvements in the microscope. Only ten years before Sharpey, discovering that eminently microscopic mechanism ciliary action, found that a simple lens was a much more trustworthy tool than the then compound microscope. But in the ten years a great change had taken place, and during the latter part, especially, of the decennium, improved instruments yielded a rich harvest of discovery in animal and vegetable life. Prominent among the new additions to truth was increased knowledge of the mammalian ovum, in acquiring which Wharton Jones, Huxley's teacher at Charing Cross, did much. But the most momentous and epoch-making step was the promulgation of the cell-theory by Schwann and Schleiden as the decennium drew to its close, and more or less connected with that step was the accurate description by von Mohl of the structure of the vegetable cell, and his introduction of the word, which, next to the word cell, has perhaps had the most profound influence on the progress of biologic science—I mean the word protoplasm.

Of this wide field of general biologic knowledge the College of Surgeons at that time took no heed, or at least made no formal demand. It is true that part of it found its place in the lectures on Anatomy and Physiology, and in the consequent examinations, but only a small part. It is also true that the lecturer on Materia Medica had by custom license to roam over almost the whole of nature, and the student in learning the nature and use of drugs took doses of heterogeneous natural history; the mention, for instance, in the Pharmacopœia of Castoreum being made the occasion of a long disquisition on the biology of the beaver.

But in this the end in view was the acquisition of facts, not training in scientific conceptions and ways of thought.

The botany, it is true, which unasked for by the College of Surgeons, was insisted upon by the Company of Apothecaries, though made compulsory on utilitarian grounds, as an appendage to and introduction to the Pharmacopœia, did serve the student in an educational way, teaching him how to appreciate likenesses and differences, even small ones, and how to distinguish between real and superficial resemblances. But the time he spent on this was too brief to make it—save in cases where a special enthusiasm stepped in—of any notable effect.

Of the then conditions of that biologic science which comes closest to the profession of physiology, I will venture to say a few words, though I will strive to curb my natural tendency to dwell on it at too great a length.

A great master—Johannes Müller—had a few years before written a great work, "The Outlines of Physiology," a work which the wise physiologist consults with profit even to-day, noting with admiration how a clear strong judgment may steer its way through the dangers of the unknown, and the still worse perils of the half-known. A study of that work teaches us the nature and extent of the advanced physiology, which at that day an accomplished teacher like Wharton Jones might put before an eager student like Huxley, and we may infer what the ordinary teacher put before the ordinary student, each perhaps then, as since, eager neither to give nor to take more than the statutory minimum.

When we look into the past of science, and trace out the first buddings of what afterwards grow to be umbrageous branches, it sometimes seems as if every time, and almost every year, marked an epoch; it seems as if always some one was finding out something which gathered into greatness as the following years rolled on. But even bearing this caution in mind, the end of the thirties and the beginning of the forties of the present century do seem to mark a real epoch in physiology. All along the line, accurate careful observation, quickened by the rapid growth of the cognate sciences, was taking the first steps to replace by sound views the sterile discussions and scholastic disquisitions which had hitherto formed too large a part of physiological teaching. The first steps had been taken, but the most marked advance was yet to come.

Though the observations of Beaumont had a few years before, by proving that gastric juice was a real thing, and demonstrating its properties, shown the nature of digestion in its true light, the older fermentative and other theories were not yet abandoned by all. Though the conversion of starch into sugar had been recognised, and pepsin had been discovered, the exact action of the digestive juices had yet to be learnt; that of pancreatic juice was almost unknown, and bile still reigned as the king of enteric secretions.

In the physiology of respiration the view that the carbonic acid of expired air was formed in the lungs by the oxidation of the carbon of the blood, still found strenuous support; for Johannes Müller found it necessary to argue at great length that the researches of Magnus on the gases of the blood had placed the matter in its true light. It had been suggested that the red corpuscles were in some way also special carriers of oxygen from the lungs to the tissues, but Müller could not regard this as anything more than a mere supposition.

When it is borne in mind that injection with mercury was the one method employed for tracing out the course of the lymphatics, it will be readily understood how imperfect was the then knowledge of the lymphatic system. And when it is also remembered that though Dutrochet had long before used osmosis to help in the interpretation of the movements of liquids in living tissues, the exact researches of Graham had yet to come, it will also be understood why, when questions of absorption and cognate questions of secretion came under consideration, they were dealt with as questions in such a condition are dealt even nowadays; much was said about them because little was known.

Though Poisseuille, taking up the matter where it had been left by Stephen Hales in the foregoing century, had begun, and the brothers Weber were just continuing, the work of placing our knowledge of the mechanics of the circulation on a sound and exact basis, and though the then teaching of the mechanical working of the heart did not differ widely from that of to-day, the gap which separates the then knowledge of the circulation, even in its mechanical aspects, from that which we possess to-day, is seen in all its width when I remind you that Carl Ludwig's first paper was not published until Huxley had ceased to be a student—until the year 1845. As to all that great part of the physiology of the vascular system which concerns its government by the nervous system, I will only say that in Müller's great work may be read the pages in which he deals with the conflicting opinions and indecisive observations as to whether the brain and spinal cord have any influence over the heart-beat, and in which, marshalling with logical force the arguments for and against the opinion that the blood-vessels have muscular fibres in their walls, finally decides that they have not.

In the physiology of the nervous system a momentous advance had been made some few years before, in the early thirties, by the introduction, through Marshall Hall, of the idea of reflex action. This was rapidly supplying the key to many hitherto unsolved physiological and clinical problems. The special functions of the several cranial nerves were being worked out by Majendie, Reid, and others. The former (with Flourens) was also making many experimental researches on cerebral lesions; and, in another line of inquiry, Bidder and Volkmann were preparing the way for discoveries to come by their important studies on the sympathetic system. The physiology of the senses was being vigorously pushed forward by Johannes Müller; but the reader to-day of Müller's volumes cannot but be struck with the smallness of the space (if we omit all that deals with the senses) which he allots to the nervous system, when we compare it with what is demanded in the present day. And no little part of even that limited space is taken up with a consideration of the laws of those "sympathies" which gave to the sympathetic nerves their name, but which have long since dropped out of sight.

Lastly, it must be remembered that many of the speculations of the preceding part of the century had remained barren, and many investigations had gone astray through lack of knowledge of the minuter changes which lie at the bottom of physiological events. Those minuter changes could not but lay hidden, so long as there was no adequate knowledge of minute structure. I have already referred to the improvements of the microscope taking place in the thirties, and this soon bore fruit in the rapid growth of that branch of biologic science once called general anatomy, later on microscopic anatomy, and now best known by the name of histology. It is well-nigh impossible to exaggerate the importance of a histological basis for physiological deductions; it is one of the chief means through which progress has been made, and must continue to be made. In the earlier days of physiology, the grosser features of structure forming the subject-matter of ordinary anatomy guided the observer to the solution of problems about functions; but after a while these became exhausted, having yielded up all they had to yield, and in due time their place was taken by the finer features disclosed by the microscope. These show as yet no signs of exhaustion, and we may look forward in confidence to their standing us in good stead for years to come. We may expect them to last until we pass, insensibly, from that molecular structure which makes itself known by optical changes, to that finer molecular structure which is only revealed by, and inferred from its effects, which is an outcome of the ultimate properties of matter, and which is the condition, and so the cause, of all the phenomena of life.

The early forties of the present century may be taken as marking the rapid rise of histological inquiry. It is true that, even before this, the labours of Henle had gone far; that in this country the brilliant Bowman had already (in 1840) given to the world his classic work on the structure of striated muscle, and a little later (1842) his hardly less important work on the structure of the kidney; that the sagacious Sharpey had embodied, in "Quain's Anatomy," a whole host of important histological observations; and that many others were at work. Nevertheless, one has only to remember how closely the progress of histology is bound up with the name of Kölliker, and to call to mind that Kölliker's first paper was not published until 1841, to see clearly how much of our present knowledge of histology, and all that that brings with it, has been gathered in since Wharton Jones taught it to the young Huxley.

If the gap which parts the physiological learning of that time from the learning of to-day is great, still greater is the gap in the teaching. Though at Charing Cross and in some other schools a course of physiology was given, apart from that of anatomy, this was not separately recognised by the College of Surgeons; it demanded simply a course of anatomy and physiology, of which the lion's share fell undoubtedly to anatomy.

In accordance with this, in most schools, at all events the greater part, and perhaps the sounder part of the physiology taught, was that which may be deduced from anatomical premises. Where the teacher went beyond this, he in most instances at least wandered into academical disquisitions and sterile discussions. Only in rare hands, such as those of Wharton Jones and William Sharpey, was the subject so treated as to be of any real use as a mental training for the medical student preparing his mind to view rightly biological problems. The science was not as yet sufficiently advanced to be an educational engine which could be safely entrusted to the ordinary teacher's use. And the method of teaching it, happily recognised now, which alone ensures the salutary influences of the knowledge acquired, that of following out in the laboratory the very steps along which the science has trod, was then wholly unknown. It was as a brilliant favourite pupil that young Huxley was encouraged by Wharton Jones to use the microscope himself, and study among other things the structures of hairs; he was not led to it, as one of a flock, in a practical course.

Indeed one kind of knowledge only was at that time demanded of the medical student, in such quantity and in such a way as to render the study of it a real mental training. Not in one year only of his course, but in each year—in his first, his second, and his third year—was the student, who hoped to obtain the diploma of the College, compelled to attend lectures, each course consisting not as in other subjects of seventy, but of double that number of lectures, on what was styled anatomy and physiology, but was in the main what we now call anatomy. Moreover, the student learnt even then his anatomy in the same way that he is bid to learn all other subjects now, not merely by listening to lectures, or even by witnessing formal demonstrations, but by individual labour in the laboratory, in that laboratory which we call a dissecting-room. Nowadays it may seem strange to insist that the student should be studying anatomy during all the three years of his curriculum, down to the very end of his studentship But we must admit the wisdom of it then. At that time human anatomy was the one branch of knowledge which had achieved anything like complete development, and which successive generations of able teachers had shaped into an engine of mental training of the highest value. It was then the mainstay of medical scientific teaching. It was in the dissecting-room that the student, of the time of which we are speaking, acquired the mental attitude which prepared him for the bedside. He there learnt to observe, to describe, to be accurate and exact, and the time spent there was wisely judged to be the most precious of his apprenticeship; the shaping of his mind by help of orderly arranged facts was perhaps even of greater value than the mere acquisition of the facts, important as this might be.

The authorities of the time were, I venture to repeat, in my opinion wiser in their generation in making this well-developed, adequately taught science of anatomy the backbone of the medical student's education; they were wise in making relatively little demand on the student in respect to the other sciences cognate and preparatory to medicine, the value to him of which consisted then chiefly in the facts which they embodied; they were also wise in giving him leave to defer his study of them until his knowledge of something of the needs of his future profession should have opened his eyes to the value of those sciences as mere records of facts.

I also, however, venture to think that the advance of these sciences since then has greatly changed their bearing towards the medical student, no less than towards medicine. What was wisdom in the forefathers is not necessarily wisdom in us the children. I have no wish to take advantage of the occasion of this lecture to make an excursion into the troubled land of medical education. But I feel sure—indeed I know—that I am only saying what the man whose name these lectures bear always felt, and indeed often said, when I suggest for consideration the thought that while some choice out of that advancing flood of science which is surging up around us, and all of which has some bearing on the medical profession, some choice as to what must be known by him who aspires to be the instrument of the cure and prevention of disease is rendered necessary by the struggle for existence—a decided and even narrow choice, lest the ordinary mind be drowned in the waters which it is bid to drink. In making that choice, we should remember that an attitude of mind once gained is a possession for ever, far more precious than the facts which are gathered in with toil, and flee away with ease. This should be our guiding principle in demanding of the medical student knowledge other than that of disease itself.

The usefulness, and so the success, of a doctor is largely dependent on many things which belong to the profession viewed as an art, on quickness of insight, promptness of decision, sleight of hand, charm of manner, and the like—things which cannot be taught in any school. But these are in vain unless they rest on a sound and wide knowledge of the nature of disease, on a sound and wide grasp of the science of pathology; and this can be taught. By a sound and wide grasp, I mean such a one as will enable him who has it to distinguish, as it were by insight, among the new things which almost every day brings to him that which is a solid gain, from that which is a specious fallacy. Such a grasp is only got by such a study as leads the mind beyond the facts into the very spirit of the science.

But what we call pathology is a branch—a wide and recondite branch, but still a branch of that larger science which we call physiology; it employs the same methods, but applies them to special problems. So much are the two one that it would doubtless be possible to teach pathology to one who knew no physiology; such a one would learn physiology unawares. But at a great waste of time. For physiology, in its narrower sense, being older, has become organised into an engine which can be used for leading the mind quickly and easily into the spirit and methods of true pathological inquiry. The teaching of it as an introduction to pathology is an economy of time. That, I take it, if compulsion be justifiable at all, is the justification of its being a compulsory study.

Further, the methods of physiology, in turn, are the methods of physics and of chemistry, used hand in hand with other methods special to the study of living beings, the general methods of biology. And here again it is an economy of time that the student should learn these methods each in its own science, and this is the justification for making these sciences also compulsory. But in all the regulations which are issued concerning these several ancillary sciences, this surely should be kept in view, that each science should be taught not as a scientific accomplishment of value in itself, but as a stepping-stone to professional knowledge, of value because it is the best means of bringing the student on his way to that.

(*To be continued.*)

CHEMISTRY AT THE BRITISH ASSOCIATION.

THE meeting of the Chemical Section of the British Association at Liverpool was not signalised by the announcement of any sensational discovery. Papers were, however, read on a number of the subjects which are at present occupying the attention of our foremost chemists, and it is to be hoped that the discussion on chemical education may help in attracting the attention of the public to that most important subject.

After the President's very interesting address, which, as was pointed out by Sir F. Abel, dealt with an industry of which the development had been mainly due to the labours of English chemists, many of whom worked in the immediate neighbourhood of Liverpool, the ordinary business of the Section was commenced with a paper on "Reflected Waves in the Explosion of Gases," by Prof. H. B. Dixon, E. H. Strange, and E. Graham. The rate of propagation of an explosion in a gaseous mixture can be ascertained by photographing the flash, as it passes along a short glass tube, on a sensitive film revolving at a known rate, and then measuring the angle through which the image has been rotated. A number of photographs of this kind were exhibited. They reveal the existence of a second wave, which passes back along the tube in the opposite direction to the flash, and at a much slower rate. This wave is probably set up by the explosion wave when it reaches the end of the tube, and by measuring its velocity the authors are enabled to estimate the maximum temperature of the gases immediately in the wake of the explosion wave. The maximum temperatures, obtained with a number of different mixtures, lie between 3000° and 4000°, and are thus of the same order as those found by Bunsen, by Berthelot, and by Mallard and Le Chatelier for the temperature of the explosion itself.

Sir G. G. Stokes expressed the opinion that the luminosity which accompanied the reflected wave might be due, not to any chemical action, but to the temporary compression of the gases, which had only cooled slightly below their point of luminosity.

The only paper on the subject of the Röntgen rays which found its way into the Chemical Section was one in which Dr. J. H. Gladstone and Mr. W. Hibbert drew a contrast between the action of metals and their salts on ordinary light and on the new rays. All the metals, except in exceedingly thin films, are opaque to light, whilst their compounds with electronegative radicles—the metallic salts—are transparent, or only exhibit a selective absorption. With the Röntgen rays the relations are quite different. The metals exhibit all degrees of opacity towards these rays, lithium being almost transparent, platinum and gold practically opaque, whilst the opacity of the other metals seems to follow the order of their atomic weights. In the salts the metals seem to retain their own absorptive power, and the absorption of a solution of a salt appears to be the sum of the absorptions of the metal, the acid radicle, and the solvent.

A paper on the "Limiting Explosive Proportions of Acetylene, and Detection and Measurement of this Gas in the Air," was read by Prof. F. Clowes. The possibility of the introduction of acetylene as an illuminant renders a knowledge of these factors of considerable practical importance. The detection and estimation of the gas in air can be carried out by the well-known flame-cap test, so small a proportion as 0·25 per cent. being readily distinguishable. A convenient portable apparatus was exhibited for carrying out the test at any desired place. All mixtures of air and acetylene which contain from 3–82 per cent. of the latter are explosive, this being a wider range of explosibility than is shown by any other gas. Carbon is deposited during the combustion of all mixtures containing more than 22 per cent. of acetylene. In a later communication the author showed that the flame cap test can also be applied to the detec-